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Damage assessments—estimation methods and sampling design

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Introduction

Quelea damage estimation is important for two main reasons. Firstly, it is the necessary initial step of problem definition before management strategies can properly be applied (Dyer and Ward 1977). Secondly, it provides the means to evaluate the success of control operations or other damage avoidance techniques in terms of crops saved (Elliott 1981c). In common with bird damage elsewhere in the world (Chapter 3), quelea damage is extremely variable in space and time, presenting many difficulties to the field worker attempting to obtain realistic and statistically valid damage estimates. This chapter reviews these difficulties and makes suggestions as to how they may be overcome.

Damage assessment methods

There are two components to any estimation method of crop loss: sampling design and damage assessment. Both must be statistically sound to produce useful final estimates. Sampling design determines sample size and sampling locality. It may involve a single stage, as in locating random points in an experimental unit, or several stages, as in choosing geographical regions of a country, then choosing fields within these regions, and, finally, sampling points within the fields (Stickley *et al.* 1979b). Damage assessment is what is done once the sampling points are reached and may have several components, e.g. measurements taken on the plant and on the size and shape of the sample plot, if plot sampling is being used, and a search strategy, if distance sampling is being used.

All assessment methods are to some degree subjective, depending on the quality of the decisions made by the assessor. These decisions will include precise measurements with a tool or the eye, and accurate discrimination

between bird damage and other causes of losses. Assessor bias, defined as a consistent deviation between true loss and recorded loss, can best be eliminated by training. Jaeger and Erickson (1980) used correction factors for individuals after they had been through a training programme for quelea-damage-to-sorghum surveys. Alternatively, assessors can be randomly assigned to sampling sites so that bias is not associated with a particular area (LeClerg 1971).

The three parameters commonly used to describe crop damage are percentage loss, absolute loss, and incidence (the percentage of plants damaged more than a stated threshold value). The optimum sampling design may differ according to which parameter is of primary interest (Otis *et al.* 1983). Although the recorded data can be either continuous (e.g. area of seed removed, weight) or discrete (e.g. a percentage scale with intervals of 10 per cent), the same principles and suggestions set forth in this chapter apply. Efforts should be made to record data on a continuous scale, since these data will contain maximum information. In those instances where a discrete scale is used (most often in visual estimation of percentage loss), it is wise to strive for as many intervals as possible, while at the same time maintain a level of practicality and reality.

Plot size and shape

Quelea damage assessment methods usually have used plot sampling to produce loss estimates; all plants within one or more sample plots within the field are assessed. The plots may be of fixed size (e.g. 1 m²) or may vary in size, as when a fixed number of plants is sampled at each location (Bruggers and Ruelle 1981; Jaeger and Erickson 1980; Kitonyo and Allan 1979). Jackson (1979) made recommendations for sample size based on numbers of plants per hectare as opposed to a number of sampling points per hectare. He did not mention the need to record plot sizes at each sampling point. Without knowing the actual area sampled, it is impossible to estimate directly the yield of the sampled field. Therefore, these considerations lead to the recommendation that damage assessment methods should use sampling plots of fixed size.

Plots can be of different shapes, the most obvious being rectangular, square, or circular. Although sampling efficiency influences this choice (Ghosh 1945), the most logical, practical plot shape would be rectangular. When sampling row crops, a rectangular plot is easily defined in terms of row length \times row spacing. For example, the sampling plot is defined as those plants contained in a portion of a row K metres long; the actual size of the plot is then $K \times$ row spacing. (This plot will be referred to as a K -row metre-plot.)

Extensive statistical literature exists on determining optimum plot size

(Cochran 1977; Federer 1955), but these theories have rarely been applied to sampling situations in vertebrate pest research. Perhaps the most straightforward approach is to conduct damage assessments for several different values of K . Variances of estimates can then be used, together with time:cost factors (amount of time required to sample different plot sizes), to calculate the relative net precision of each method. Cochran (1977: 234-6, 243-4) described two methods for handling such data and presented an example. Otis *et al.* (1983) used a similar approach to determine plot size for sampling bird damage to sprouting rice and found smaller plots to be preferable. Generally, I believe the smaller the plot size the better, particularly in those situations in which damage is likely to be clumped or heterogeneous in the field, and the size of the clumps is large relative to practical plot size. It usually is best to sample many small plots in a field rather than a few large ones.

Crop types

Rice Most field surveys of quelea damage to rice have estimated percentage loss from the difference in average weight between damaged and undamaged panicles collected in plots (Bruggers and Ruelle 1981; Jackson 1979). This approach requires that the percentage of damaged panicles be used to adjust the difference in average weight. This method is subject to a potential bias because the assumption must be made that birds select and damage panicles at random. If, in fact, birds do show some preference for larger and heavier panicles, then there will be a sampling bias resulting in underestimation of loss. Comparison of the histograms of variables such as length or area of head on selected (damaged) and unselected (undamaged) heads (such variables being correlated with weight, yet unchanged by the occurrence of damage), will reveal if a bias exists (L. McDonald, pers. comm.). The bias could be avoided by comparing weights of random plots exposed to birds and random plots that have been protected, usually by use of exclosures (Bruggers *et al.* 1981b). However, such a procedure is totally impractical in large-scale surveys, and in most situations is not feasible even for small experiments.

Although bird damage research on ripening rice in the US is over 50 years old (Kalmbach 1937), reliable and standardized methods have not been developed (Meanley 1971). Recently, F. Crase and R. DeHaven (pers. comm.) evaluated seven assessment methods and concluded that either visual estimates of percentage loss, or estimates based on actual counts of missing and remaining spikelets were most feasible. P. Lefebvre (pers. comm.) recommended that visual estimates by trained assessors be given further consideration for large-scale surveys, and that the only practical alternative for use in small-scale experimental work may simply be incidence of damaged panicles.

For quelea, it appears that there are two available choices for rice damage estimates:

- (1) If data are available that indicate random selection of panicles by birds, comparison of weights of damaged and undamaged panicles can be used. The appropriate formula is:

Estimated per cent loss =

$$1 - \left(\frac{\text{Av. wt(g) damaged panicles}}{\text{Av. wt(g) undamaged panicles}} \right) \times \left(\frac{\text{No. damaged panicles}}{\text{Total no. sampled panicles}} \right)$$

A minimum of 50 row centimetres should be used as the plot size.

- (2) If assessors can be trained visually to estimate percentage loss, then use of this simplest assessment method is justifiable. If training is impractical and assessor accuracy difficult to verify, this method cannot be recommended. Minimum plot size can be reduced to perhaps 20 row centimetres. In addition, a random sample of plot weights should be taken from the field so that a direct estimate of yield is available, if the actual yield cannot be obtained from the farmer.

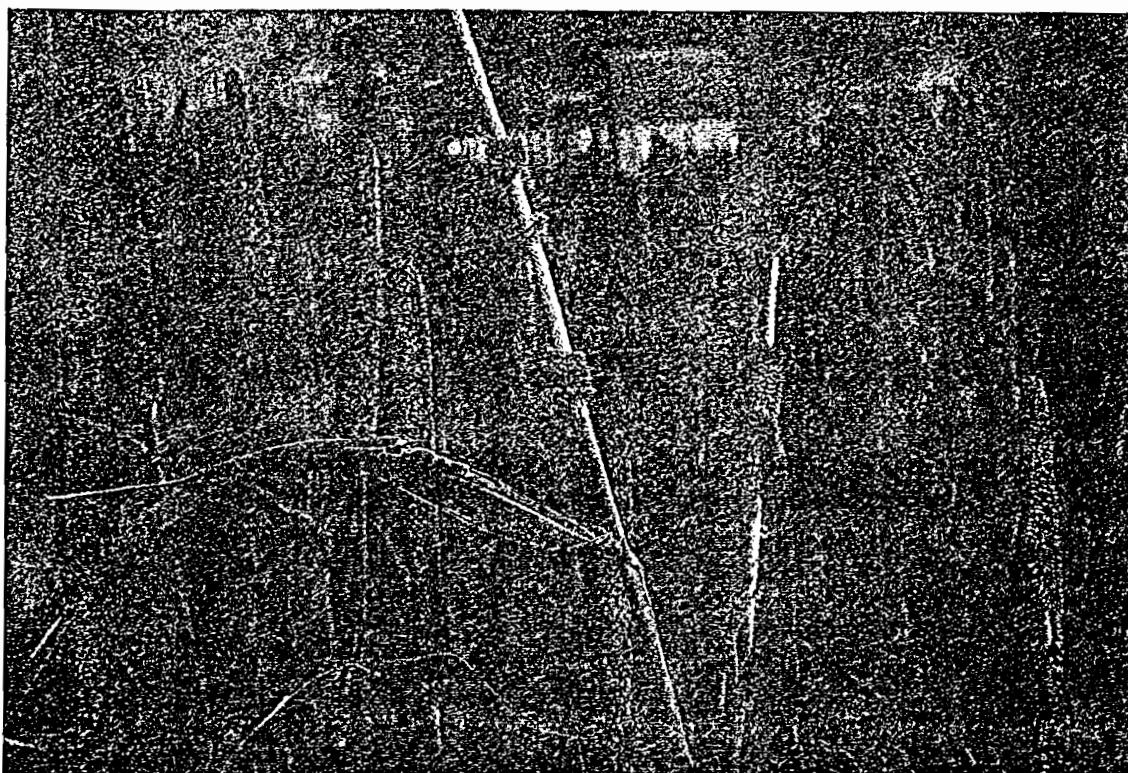


Fig. 8.1. Damage in bullrush millet can be assessed by measuring the length of the panicle eaten relative to the total length of the panicle, and extrapolating to the amount of grain lost (photo: R. Bruggers).

Millet The most accurate technique developed thus far to evaluate millet damage by quelea measures the length of damage on four axes of the ear and compares the average length of damage to the length of the total ear to estimate percentage loss (Manikowski and Da Camara-Smeets 1979a). These authors also developed a prediction equation to estimate percentage loss from incidence of damaged heads only, but state that such a technique would probably only be useful in planning or preliminary studies, and not in detailed, presumably experimental work. Considering the relatively easy length measurement technique, this procedure, with minor modifications, would be appropriate in all millet damage research (Fig. 8.1).

The accuracy of this method depends on the assumption that the width of the length of damage measured is fully one-quarter of the head's circumference. If the average width of the damage is less, then percentage loss is overestimated. To avoid this potential bias, it is necessary to measure the average width of damage along each axis and the circumference of the head. Then an estimate of percentage loss becomes:

$$\text{Estimated per cent loss} = \sum_{i=1}^4 w_i l_i / WL$$

where

w_i = average width of damage on the i th axis

l_i = length of damage on the i th axis

W = circumference of the head

L = length of the head.

In the absence of real data on the efficiency of various plot sizes, any recommendation is *ad hoc*, but I suggest that for millet, plot sizes averaging approximately five plants per plot be used.

Wheat Most bird damage surveys in wheat in Africa have used weights of damaged versus undamaged panicles (Elliott and Beesley 1980; Kitonyo and Allan 1979). The same potential bias described for rice is applicable for wheat. Allan (1975) visually classified heads into five categories of per cent loss. Dawson (1970) estimated wheat damage in New Zealand by actually counting damaged and undamaged seeds on each sampled head, but stated that accurate counts of missing seeds were difficult because 'the bracts tend to hide the gaps and the number of grains per spikelet varies'. This observation implies that visual estimates could be subject to significant negative bias. Dawson (1970) accommodated the difficulty of counting missing seeds in the presence of a gap by estimating numbers of seeds present on adjacent spikelets, a method requiring 1 h to assess approximately 75 heads, even with

low bird damage. Therefore, as with rice, counting missing and remaining seeds appears impractical.

Comparing weights and visual assessments seem to be the best techniques for wheat. If a visual method is used, the plot size used could be less than 20 row centimetres, because Dawson (1970) concluded that sampling four plants per sampling plot was the most time-efficient method. Also, if damage categories are used, I suggest that there be at least 10 categories. The necessary training programme for assessors should, therefore, strive for accuracy with this degree of refinement.

Sorghum Most surveys of quelea damage to sorghum have used a visual estimation method (Bruggers and Ruelle 1981; Jackson 1979). Assessors can be trained by use of known-damage heads (Jackson 1979). Alternative methods have been investigated. D. Otis (unpubl. data) protected sorghum heads from bird damage in the US by enclosing them in paper bags and comparing the yields of protected plots with paired, unprotected plots. Yield differences were quite variable, necessitating large sample sizes to achieve reasonable precision. This approach would probably not be feasible in large surveys, because a trip to each field to be sampled is necessary before the onset of bird damage. A linear regression equation for predicting percentage loss from incidence of individual damaged heads was developed by Manikowski and Da Camara-Smeets (1979a), but their subsequent discussion states that the amount of error in the model precludes its use in experimental studies. I remain sceptical of indirect approaches such as theirs because the method is usually developed from studies conducted in one general location in 1 year, and the consistency of the relationship over space and time is not verified.

For experimental research, visual estimations made by trained assessors probably is the most viable approach (Fig. 8.2). Visual estimates should also be used in large surveys, if it is believed that the behavioural feeding patterns of quelea could be different from those investigated by Manikowski and Da Camara-Smeets (1979a). Different feeding patterns and, hence, different quantitative relationships between incidence and per cent loss, could be caused by variation in crop culture, weather, habitat, and other factors.

Data on optimum plot size for sorghum apparently are not available. I suggest that a plot size large enough to contain five plants on average be used. This recommendation, as for all recommendations for row crops, assumes that plants are sampled down rows, not across rows or any other configuration. This choice is justified basically because of its simplicity and ease of plot definition.

One of the drawbacks of visual damage estimates in sorghum, as in other



Fig. 8.2. Sorghum damage in Somalia is usually measured by making a visual estimate of the percentage lost from each head (photo: R. Bruggers).

crops attacked by quelea, is that they provide no information on actual yield loss. If crop yields vary significantly among fields, and if research objectives dictate, it becomes desirable to collect data that can be used to adjust for potential yield differences. A quick, simple approach would be to devise a simple scale for rating the yield of sampled plants. This scale could be used to assign scores to each sampled plant, which could then be used to weight the estimates of percentage loss. For example, loss on higher yielding plants would receive more weight in the final estimate of loss for the field than would loss to low-yielding plants. An example of this technique is illustrated in the section 'Large-scale survey design.'

Field sampling design

In field sampling design, a field is defined as a sharply delimited, continuous planting of grain, e.g. a 1-ha experimental unit in a protection trial, a 0.24-ha plot within a larger planting, or a 100-ha commercial planting. The assessment technique is influenced by the size of the area to be sampled and the precision required of the results. Our studies on blackbird damage to sprouting rice (*Otis et al.* 1983) showed that the variance contributed by an individual field to the overall variance of the percentage damaged for an entire area was negligible compared to the variance in loss among fields. We

chose, therefore, not to sample individual fields heavily but to sample as many fields as possible.

Precision of loss estimates for individual fields is an important consideration when designing experimental field trials. Firstly, precise loss estimates are usually necessary if compensation is to be paid to farmers for use of the experimental fields. Secondly, sampling of experimental units, as opposed to a complete census of damage within the unit, results in a loss of information, i.e. the power of the experiment in detecting differences is reduced (Yates and Zacopanay 1935). The amount of loss depends upon the relative sizes of the within unit variation in damage (sampling error) and the between unit variation (experimental error), and the optimum choice for numbers of experimental units versus level of sampling in each unit can be determined if preliminary information is available (Federer 1955). In the typical experimental situation in Africa, experiments must be performed with very limited resources (Bruggers and Jackson 1981), including numbers of experimental units available. Often, the researcher will not have the flexibility to adjust the number of experimental units based on expectations of the relative sizes of sampling and experimental error. The necessary requirements of adequate bird numbers and farmer co-operation severely restrict the range of choices for number of units. Therefore, we should attempt to minimize, within practical constraints, that component of the experimental error due to variance in individual unit estimates.

The above considerations lead to the following three general principles of field sampling designs. All designs subsequently considered in the chapter adhere to these principles.

Principle: The level of sampling effort in individual fields will be greater in an experimental context than in a large survey context.

Principle: It should be possible, at least conceptually, to construct a list or 'frame' of every sampling unit (plot) in the population (field).

For example, if a sorghum field is to be sampled, using one row metre as the sampling unit and the field has 100 rows, each 50 m long, then the description of each sampling unit in the field could conceivably be written down. There would be 5000 such sampling units, beginning with the first metre in Row 1 and ending with the last metre in Row 100. Of course, it is not necessary to do this exercise in practice, but it is helpful to be able to think of the field as a collection of definable sampling units when constructing a design that specifies how units are to be chosen.

Principle: Each sampling unit in the population, and therefore, every plant in the field must have a positive (not necessarily equal) probability of being

chosen for assessment. This property is guaranteed and the probabilities determined by a specified method of random selection of the units.

This principle is the foundation of probability sampling. It allows measures of variability of estimates, such as standard errors, to be calculated from the sample data, and hence, leads to valid statistical inferences concerning the population sampled. Alternatives such as 'haphazard' or 'expert choice' sampling depend on the validity of broad assumptions about damage distribution which are difficult to evaluate.

Field sampling in large-scale surveys

Both the suggested field sampling designs and the actual methods used in surveys of quelea damage have been diverse. They vary from the random selection of points from a grid superimposed on the field (Anon. 1979; Kitonyo and Allan 1979) to sampling at regular intervals along diagonals (Church 1971) or along transects placed to cross the field adequately (Jackson 1979; Jaeger and Erickson 1980) or systematically (Bruggers and Ruelle 1981) (Fig. 8.3). If neither the initial transect location nor the starting location along transects is randomized, the design will not result in every

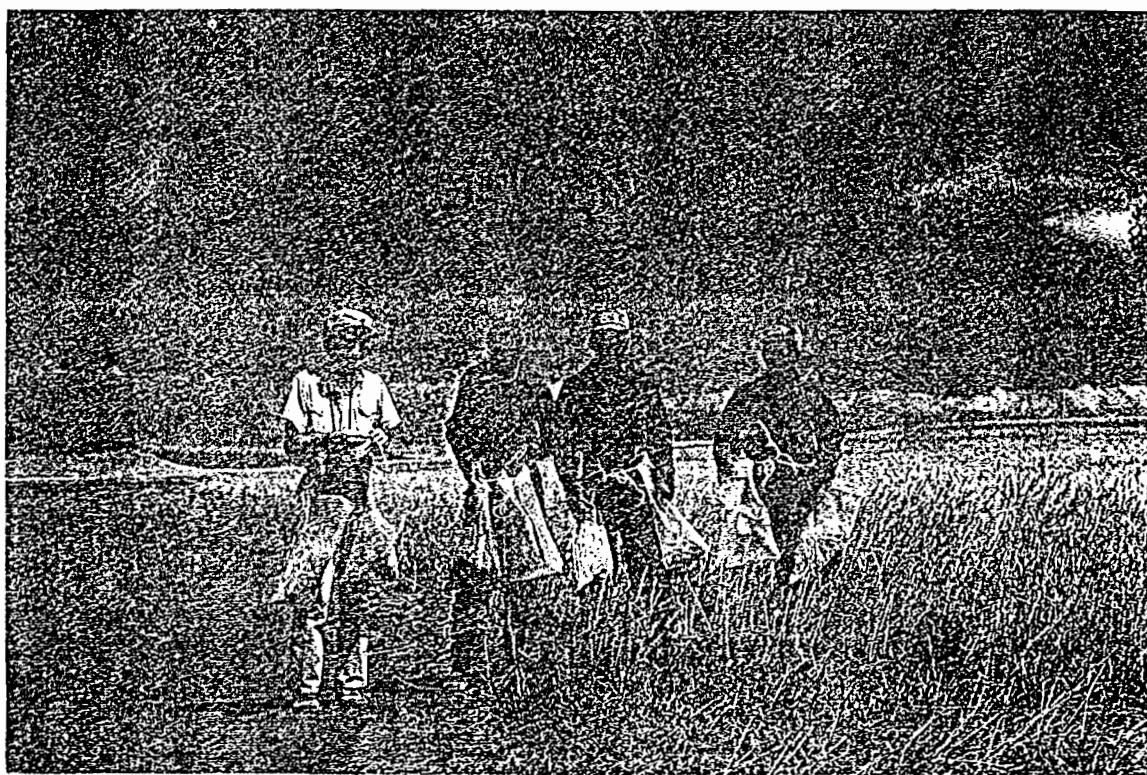


Fig. 8.3. Wheat damage is assessed in West Kilimanjaro, Tanzania, by cutting samples and comparing the yield of damaged spikes relative to the yield of undamaged spikes (photo: M-T. Elliott).

portion of the crop having a positive probability of being sampled. These designs, therefore, violate the principle of probability sampling.

For large-scale surveys, the correct procedure would be as follows (see also Fig. 8.4):

- (1) Construct a rough diagram of the field and establish a baseline, preferably perpendicular to the direction of rows.
- (2) Divide the estimated length of the baseline, e.g. L , by 4, and let the result be denoted W and select a random number between 1 and W , e.g. R . Four transects running perpendicular to the baseline, i.e. along rows, will be sampled at distances of R , $R + W$, $R + 2(W)$ and $R + 3(W)$ along the baseline. Simply stated, four rows, located at regular intervals of W starting from a random point R , are chosen to be sampled.
- (3) Estimate the total length of the four rows to be sampled, say M , and divide by the number of samples to be taken, say N . Choose a random number between 1 and M/N , say T , and take the first sample a distance T up the first row. Continue to take samples at regular intervals of M/N . When the end of a row is reached, simply continue the count to the next row.

Of course, the question still remains of how large the number of sample plots (N) should be. A specific answer is difficult in the absence of any

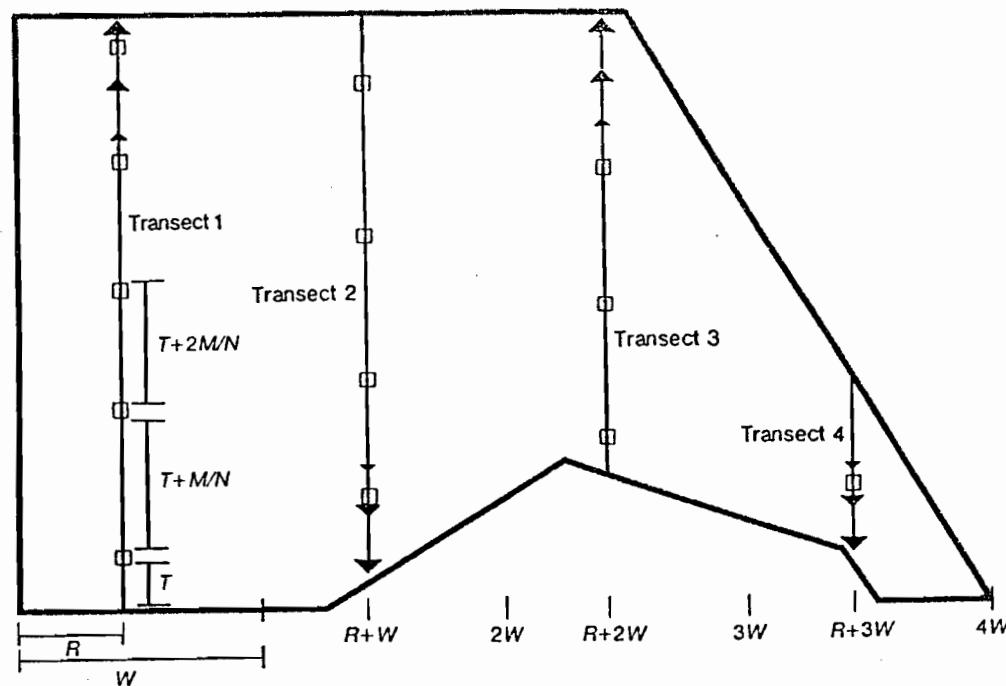


Fig. 8.4. Illustration of the placement of four transects and sample plots (□) across the region of interest. Note that the transects are parallel and equally spaced, but have different lengths due to the irregular shape of the region.

preliminary estimates of distribution or variation of losses, but I suggest a range of 20–60 sampling locations of any size per field. The reasoning is to devote more effort for the given amount of manpower available to increasing the number of fields that are sampled and therefore, to minimize (within reasonable limits) the amount of sampling done within a field. Modification in this sampling design will undoubtedly be necessary as specific applications are encountered. The researcher should strive to ensure that such changes in design do not result in violations of the basic principles of sampling design that have been discussed.

Field sampling in experimental trials

In experimental work, it is more desirable to have unbiased estimators of precision for each experimental field, and therefore, a different sampling design is necessary. The size of the experimental unit is of course a major consideration. In African field trials, this size has ranged from <1 ha (Bruggers 1979a,b; Funmilayo and Akande 1977), to 2 or 3 ha (Martin 1976) to occasional use of large (30–40 ha) fields (Bruggers 1979a). As a generalization, Bruggers and Jackson (1981) stated that experimental trials in developing countries are often conducted at agricultural research stations with experimental units of <0.25 ha. In experiments with units of ≤ 1 ha, a simple sampling design should be used for locating sample plots. The steps to construct this simple random design are:

- (1) Construct a diagram of the field. If the field is irregularly shaped, circumscribe the field with a rectangle or a simple polygon.
- (2) Select N random locations (row, pace) in the field and sample damage at these locations. If the field is irregularly shaped, it is convenient to select a few extra locations. Once in the field the assessor may discover that some of the samples fall outside of the field boundaries due to inexactness in the field diagram. When this occurs, the extra random locations can be substituted.

The above design would obviously be too time-consuming in larger fields. In addition, when placing a relatively small number of locations completely at random in a large area, a satisfactorily uniform coverage of the field may not be achieved. To guard against this possibility, and possibly to increase efficiency of sampling effort, stratification can be used.

In stratified random sampling, the population (field) is divided into mutually exclusive and exhaustive parts called strata, and a random sample of plots is taken within each stratum. Efficiency is improved if sample plot damage within the individual strata is more homogeneous than the entire population of sample plots. Strata boundaries are established at the dis-

cretion of the researcher, but I suggest that there be from two to five strata. Populations may be stratified by simply splitting the field into equal areas to ensure uniform coverage or by preliminary guesses of where the most variation in damage in the field is expected, and then establishing strata in which low, moderate, and high variation (which is usually proportional to the amount of loss) is expected. I would recommend that stratification schemes be kept simple with the major objective of achieving more uniform coverage. Within each stratum, sampling locations could be located randomly if the individual strata are sufficiently small or along at least four transects (rows). However, transect locations along the baseline should be selected randomly and not systematically. (Random selection will permit valid estimation of variance of the loss estimate.) An example of this sampling design is illustrated in Fig. 8.5.

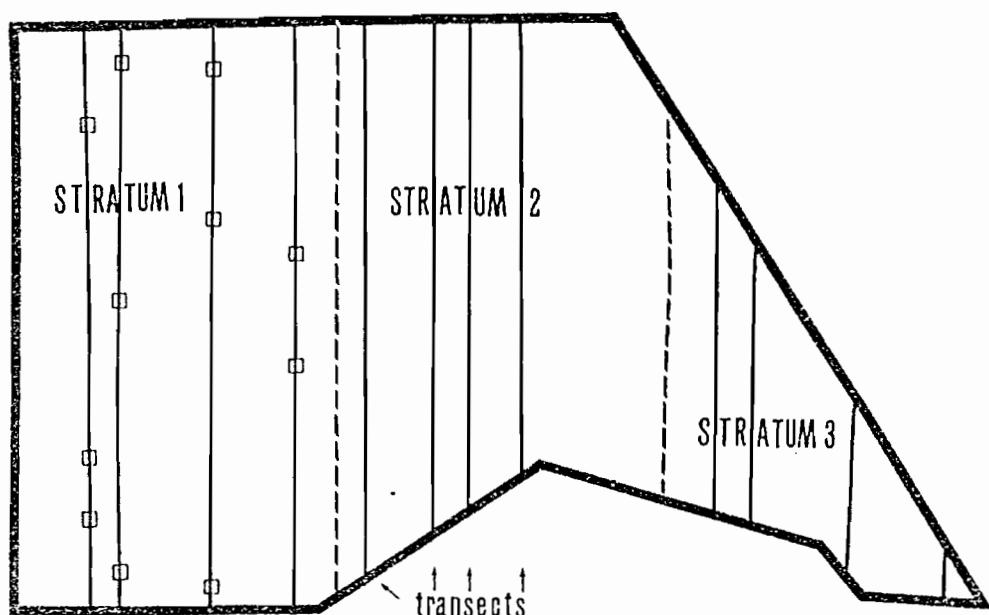


Fig. 8.5. Illustration of the use of stratification. The field has been divided into three strata of equal width but unequal area due to the irregular shape of the field. Within each stratum, four random locations for transects are selected along the baseline. Sample plots (□) are located at random on the transects.

A third sampling strategy involves the technique of post-stratification (Cochran 1977:134). This approach could be used when the original sampling locations are placed in the field at random and without stratification. After data have been collected, the researcher may be able to examine the damage pattern and delineate boundaries that produce strata which are homogeneous within. It is important for the researcher to know the exact size of the resulting strata. These exact strata sizes can then be used as weights, as if stratification had been used originally. For example, consider a 1-ha field in

which 50 plots have been placed at random. Upon examining the resulting damage data, the researcher concludes that damage within the outer 10 m perimeter of the field is much greater than in the interior. Therefore, the data are post-stratified into interior and perimeter strata, which receive weights of $0.64 = (100 - 2(10))^2 / 10\ 000$ and $0.36 (= 1 - 0.64)$, respectively, to calculate losses for the entire field. Note that the stratum weights do not depend upon how many plots actually fell within each of the strata. Post-stratification could be a useful way to increase precision in situations in which a few obvious, easily-defined strata can be created after examining the data.

The choice of the number of plots to be sampled (N) should guarantee the desired precision in our estimates. However, 'the decision [about N] cannot always be made satisfactorily; often we do not possess enough information to be sure that our choice of sample size is the best one' (Cochran 1977). This information essentially is a quantitative estimate of the expected variation within field damage. In the absence of previous research involving similar conditions and parameters, a somewhat arbitrary decision must be made. It has been my experience that variation in damage within any field with a significant amount of damage is large, and that sample sizes must, therefore, be relatively large, perhaps on the order of 80–100 plots. This suggestion is made with the goal of achieving a coefficient of variation of 10–15 per cent. With some standard available, as well as previously collected data, determining necessary sample size (and perhaps changes in sampling design) can be made accurately and objectively. Good sampling designs are the result of a sequential process of data collection.

Sample size recommendations are independent of field size. This is contrary to many researchers' intuition; in fact, sample size recommendations are often made in terms of plots per hectare or some other unit of measurement (Jackson 1979). However, in this context, the most important parameter in determining N is the variation in damage among the sampling units in the population (field) and not the size of the population. Usually the number of potential sampling units in any field is practically infinite. In a 2-ha field with 1-m row spacing, there are 100 000 sampling units of 0.2 m^2 (20 row centimetres). Thus, for any practical sample size N , the sampling fraction ($N/100\ 000$) will be negligible, i.e. very close to zero. In a field one-half as large, this fraction will remain essentially zero; thus, variances of the estimates will be the same in both instances, given that the variation among sampling units is the same in both fields. For example, using eleven 0.37-ha experimental units of sunflower planted in Sand Lake, South Dakota, C.E. Knittle (unpubl. data) evaluated the efficacy of several treatment forms of Mesurol^R in repelling Red-winged Blackbirds *Agelaius phoeniceus*. In each unit, twenty 5-head plots were located using a simple random sampling design, and square centimetres of seed removed were measured using a

plastic template. Below is the average loss (\bar{x} , cm²) per plot and the coefficients of variation (CV) for each unit, assuming a fixed plot size:

| Experimental unit | \bar{x} (cm ²) | CV(\bar{x}) (%) |
|-------------------|------------------------------|---------------------|
| 1 | 1365.90 | 9.50 |
| 2 | 1125.35 | 8.86 |
| 3 | 1259.35 | 7.14 |
| 4 | 1157.35 | 6.55 |
| 5 | 1132.45 | 5.59 |
| 6 | 650.35 | 19.54 |
| 7 | 679.15 | 11.78 |
| 8 | 1448.15 | 8.03 |
| 9 | 1049.60 | 7.51 |
| 10 | 1152.50 | 7.77 |
| 11 | 1115.80 | 5.60 |
| Average | 1103.27 | 8.90 |

These data indicate that estimates of loss for the units are of adequate precision. In fact, if the number of plots per units were reduced to 11, the average CV (\bar{x}) would increase only slightly, to an acceptable 12 per cent (calculated by solving the equation $\sqrt{\frac{20}{N}} \times (8.9) = 12.0$).

We may also examine the adequacy of sampling units from the viewpoint of the sensitivity of the analysis of variance in detecting differences among the treatments. That is, how much information (sensitivity) was lost by having to sample the plots as opposed to measuring or harvesting them completely? The key to this approach is in the ANOVA (Analysis of Variance):

| Source | DF | MS |
|--------------------|-----|----------------------------------|
| Total | 219 | |
| Treatments | 3 | 787 402 |
| Experimental error | 7 | $1\ 396\ 875 = \hat{\sigma}_e^2$ |
| Subsampling error | 209 | $179\ 672 = \hat{\sigma}_s^2$ |

The estimate of the percentage loss of information is (Federer 1955:80):

$$L = 100 \left[1 - \frac{k}{n} \right] \frac{\hat{\sigma}_s^2}{\hat{\sigma}_e^2}$$

where k = number of sample plots/unit and n = total number of plots available in the unit. Stated differently, the number k/n merely represents the proportion of the unit that was sampled. This fraction is almost always very close to zero; thus, an approximation to L may be taken as $100 \times \hat{\sigma}_s^2/\hat{\sigma}_e^2 = (100) (179\ 672)/1\ 396\ 875 = 12.7\%$. Thus, about one-eighth of the information contained in the experiment was lost due to sampling of the units. If the same rate of sampling is maintained in future experiments, the size of the experiment (number of experimental units) would have to be increased by a factor of $1.145 = 1/(1-0.127)$, relative to an experiment in which complete enumeration of the plots was planned. Finally, it can be shown that if the level of sampling were decreased to 11 per unit, as suggested above, the corresponding information loss would increase from 12.7 to 20.5 per cent. This is probably an acceptably small increase, considering the savings in manpower associated by nearly halving the amount of damage assessment required.

Large-scale survey design

Direct assessment

The most challenging of the sampling design problems encountered in quelea research is that of constructing a design for surveys over large geographical areas. The objective is to produce an unbiased picture of the level and distribution of damage in the area and to obtain estimates of the precision with which the losses are assessed. As with any sampling problem, a design is easily created given unlimited manpower, but of course the situation in Africa is one of very limited manpower and a plethora of logistical problems. These obstacles have, unfortunately, led some researchers to discard the above objective. Ash (1981) stated that 'the gathering of crop damage data is tremendously dull work, very time consuming, and no one in Somalia has much interest in undertaking it'. Elliott's (1981c) opinion was that 'a definitive, fixed assessment of the problem in East Africa is not considered possible' because of variations in climate and rapidly changing agricultural practice. As a substitute for objective surveys, extrapolations of farmers' opinions or of a limited tour of some known damaged areas have been used to assess the magnitude of the problem. I agree with Lenton (1981) who, in talking about his estimates of crop loss in Sudan derived from sketchy data,

stated that 'until good objective damage assessment data are produced over wide areas many of these estimates of loss are meaningless.' Moreover, such substitutes, even if they produce reasonably accurate estimates of country-wide crop losses, do not provide much information on damage distribution, which is often of more interest.

In spite of all the difficulties, several large-scale objective surveys of quelea damage have been conducted, each with its own particular survey design (Plate 7). In Kenya, Kitonyo and Allan (1979) used 0.5° grid maps (55 km^2) and Land Registration numbers to construct a sampling frame of wheat farms. They then selected farms in proportion to the acreage planted in the 0.5° blocks making up the defined region of interest. Allan (1980) subsequently altered this scheme by using the political division of a ward instead of 0.5° blocks. Both of these designs seem to satisfy the need for a theoretical sampling frame from which to randomly select fields, a defined geographical area (population) of interest, and a known, positive probability of selection for each field.

Thus, using appropriate estimation formulas (their design is really a two-stage cluster design), valid estimates of loss and associated precision could be calculated. In 1978, Bruggers (1980) assessed crop losses in Somalia using a network of randomly located sampling locations on government and private farms. He later stated, however, that the resulting figures should be used cautiously since 'it is uncertain whether the area sampled is representative of the district'. Implied is the fact that all fields did not have a chance to be sampled and, therefore, valid inferences about district losses are not possible. Cereal crops in Senegal were surveyed by Bruggers and Ruelle (1981). Fields in all crop-growing regions were sampled at systematic distances along paved or dirt roads (although the distance between stops actually varied somewhat based on finding a mature crop). Usually, only fields within 500 m of the road were sampled, so that inferences to the entire crop are not possible. As a result, it is difficult to assign a probability of selection to a field (a sampling frame does not exist) and, therefore, to construct the appropriate estimation formulas.

I wish to emphasize that the method of field selection (i.e. the survey design) determines how loss estimates are actually calculated. There is no one formula (e.g. the average of all sampled fields) that is appropriate for every design. Therefore, it is important that the investigator is aware of the type of design actually being used (e.g. cluster, stratified, multi-staged) and knows the proper formulae associated with such a design.

With this brief review of past surveys, let us consider the essential properties of a properly designed survey:

- (1) The population for which estimates of crop loss (or any other parameter) are desired must be clearly defined. In this context, a population could be a

country, a region, or a type of farm (e.g. governmental). Notice that this property is closely related to an earlier stated principle involving conceptualization of a sampling frame. That is, once the population of interest is defined, a theoretical listing of all the members (fields) of the population is possible.

(2) Clear objectives for the survey should be identified, usually in terms of desired loss estimates in an area or in several areas, e.g. estimates of dollar loss in the country, of the percentage of fields with any quelea damage in each of several regions, of kg/ha loss on private farms, etc. A second type of objective, statistical in nature, must also be specified in terms of the desired reliability of the estimates, i.e. it is desirable to have estimated per cent loss with standard errors of less than 2 per cent, or estimates of dollars lost in a region within limits of \$500 000.

(3) After points (1) and (2) have been established, the most critical element of the planning process occurs. An estimate must be made of the cost, in terms of manpower and money, associated with achieving the desired objectives. If the resources available are not sufficient, then either the objectives must be revised or the project dropped. The point to be made is that if the survey cannot be done well, with a reasonable chance of satisfying the stated objectives, then it is not worth doing. Effort has been mostly wasted if, upon completion of the survey, estimates of reliability of the loss figures are not available, or extrapolation to the population of real interest is not valid. Of course, cost will vary with the proposed sampling design, both field and survey, but the range in cost will probably be small compared to the average cost.

(4) Choice of the survey design should be influenced by both the objectives of the survey and the availability of resources. A variety of questions must be answered. If regional estimates are desired, what are the boundaries of these regions, and how shall effort be allocated to the strata within regions? Does the within field sampling design require a team of assessors, or can individuals handle the sampling alone? What type of statistics concerning the amount and distribution of the crop are available to help define sampling units at each stage of the design? What are the appropriate estimation formulae? Of paramount importance is that a probability sample of the fields (or whatever the basic sampling unit is) is achieved. Ultimately, every plant of every field in the population of interest must have a chance to be sampled.

The following example of survey design is a summary of the methodology proposed by Otis (1984) for surveying relatively large sorghum-growing areas in Tanzania. Discussion of this design will serve to illustrate the application of principles stated above.

The primary objective was to estimate the number of hectares of sorghum lost to birds in a large ($\approx 10\ 000 \text{ km}^2$) region of central Tanzania. Most cultivation in this area consists of small, scattered, subsistence farm holdings. Because no statistics were available on the amount and distribution of cultivation, it was decided that the stated objective would require two different, independent surveys. The first was to estimate the hectarage of sorghum cultivation and to create a crude map of the distribution of crops within the region. The second would produce an estimate of the percentage of sorghum lost to birds; the product of this estimate and the number of available hectares produces an estimate of the number of hectares lost. Let us consider the design of these two surveys in more detail. The aerial survey described was conducted, but no data were collected using the ground survey design. Therefore, this part of the example is hypothetical.

A fixed-wing aircraft, capable of carrying at least three passengers and maintaining a speed of 200 km/h, was used to produce a survey estimate of hectarage. The idea was to fly a series of parallel transects across the area at an altitude of 100 m. One passenger served as a navigator, helping the pilot to locate landmarks, time the length of transects, and time the counts of two observers seated on either side of the aircraft. A small sighting guide, such as a 0.5-cm circle, was placed on each observer's window in a position that allowed the observer, at any given instant, to fix a location on the ground. In effect, the eye was taking a snapshot of a point on the ground. Once the navigator determined that the aircraft was on the transect, he signalled the observers in 15-s intervals until the transect was completed. At each signal, each observer took a sighting and recorded a 1 if the sight was on maturing sorghum at that instant, and a 0 otherwise. The resulting sequence of 1's and 0's for each observer for each transect represented the basic data collected in the survey.

At the planning stage, we had to define precisely the area to be surveyed and we chose to sample $4.5^\circ \times 0.5^\circ$ blocks within the Singida region. To determine transect locations, a baseline was established along one side of each block. Transects were flown perpendicular to this baseline that was divided into N intervals of equal width, e.g. W , and N was the number of transects flown. For each block, a random number between 0 and W , e.g. R , was chosen and the first transect was begun at this distance down the baseline from the starting corner. Subsequent transects were flown at equally spaced intervals at distances $R + W$, $R + 2W$, ..., $R + (N - 1)W$.

The appropriate formulae for estimating the number of hectares in sorghum cultivation within each block, and its associated variance, require the following notational definitions:

N = number of transects;

M_i = number of observations taken by each observer on the i th transect, $i = 1, 2, \dots, N$;

- L_i = length of the i th transect, $i = 1, 2, \dots, N$;
 A = area, in hectares, of the sampled region; and
 b_i = total number of 1's recorded by both observers on the i th transect, $i = 1, 2, \dots, N$.

The estimate of the proportion of the i th transect devoted to sorghum cultivation is then $\hat{P}_i = b_i/2M_p$, $i = 1, \dots, N$, and the estimated proportion for the block is

$$\hat{P} = \sum_{i=1}^N L_i \hat{P}_i / \sum_{i=1}^N L_i.$$

A simple estimate of the total number of hectares in sorghum in the entire block is then $A\hat{P}$, with estimated variance

$$\text{Var}(A\hat{P}) = A^2 \sum_{i=1}^N L_i (\hat{P}_i - \hat{P})^2 / \bar{L}^2 N(N-1),$$

where $\bar{L} = \sum_{i=1}^N L_i/N$.

The estimate for the total hectarage in all four blocks was obtained by adding the four individual block estimates. The variance of this estimate is the sum of the individual block variances. A rough map of the pattern of cultivation in the region was made using the ordered sequences of 1's and 0's recorded by the observers. As mentioned previously, this map can assist in the planning of the damage assessment phase of the survey.

There are two essential components to a design to estimate percentage bird loss: (1) an accurate map of the target region that contains all available information about the distribution of the crop and the locations of all possible roads, and (2) a clear idea of manpower availability and associated operating resources or the desired precision of the final survey loss estimates. Ultimately, the design will reflect a compromise based on both specifications, because rarely are both compatible. These considerations are important for determining sample size, but not to the fundamentals of the design itself. Let us first discuss the basic outline of this design.

Any random sampling design involves a random selection of sampling units, and often subsampling units, which must be conceptually defined. In large-scale crop surveys, the primary sampling unit is usually defined as a single cultivated field, and plots of a specified size, e.g. 4 m^2 , within the field are defined as the subsampling units. These definitions dictate that some method be constructed for randomly selecting specific fields. However, in Tanzania we were not concerned with the distribution of damage to individual fields and, thus, to the individual farmer, but only with the

regional loss. Therefore, the concept of a field as a primary sampling unit was inappropriate in this sampling design. Rather, the target region was viewed as a single mosaic consisting of two classifications—sorghum cultivation and other cultivation or land use. (The same concept was involved in the aerial surveys of cultivation.) The idea would be to sample this mosaic by selecting locations at random and assessing damage within a specified area around these locations. The key to specifying the sampling locations is a road system within a region. On the regional map, roads are divided into 2-km intervals, and each interval is assigned a unique number. A random selection of these intervals is then made and each of the selected intervals would be sampled for sorghum damage. Note that this method represents a simple random sample of locations within the region, and not a systematic approach, in which all roads are travelled and assessments made at equally spaced intervals. The statistical efficiency of both of these sampling methods should be approximately equal, but the recommended approach should save a substantial amount of travel time.

Survey teams of a driver and two assessors then assess damage at the identified sampling locations. When reaching the sampling location, the first assessor chooses a side of the road via a coin flip, and begins to walk a 0.5-km transect perpendicular to the direction of the road. Upon encountering sorghum cultivation along this transect, the assessor samples circular plots of 1-m radius placed at predetermined intervals. The length of this interval should range between 20 m and 50 m and vary inversely with the density of the cultivation in the region. In practice, the assessors should be allowed temporarily to leave the transect to sample cultivation within perhaps 50 m on either side. If this is done, the circular plots should be sampled in a direction parallel to the direction of the original transect and should begin at a random distance along the dimension of the cultivation that is perpendicular to the transect. If possible, the assessor should then return to the original transect after reaching the end of the cultivation. The important point is that the assessor continue until he is at a point 0.5 km in perpendicular distance from the road. He then executes a 90° turn in the direction of travel in which the vehicle approached the sampling location and walks another 0.5-km transect, proceeding to sample circular plots as described. After this transect is completed, he then executes a second 90° turn towards the road, and walks the 0.5-km transect back to the road, again sampling, as described. In the meantime, the driver will have dropped off the second assessor at approximately this same location, 0.5 km down the road from where the first assessor began. This second assessor will proceed exactly as described for the first, except on the opposite side of the road. Figure 8.6 illustrates the resulting pattern of sampled plots. Note that, in effect, the sorghum within a 1-km² block of land has been sampled at this location. Also, this description

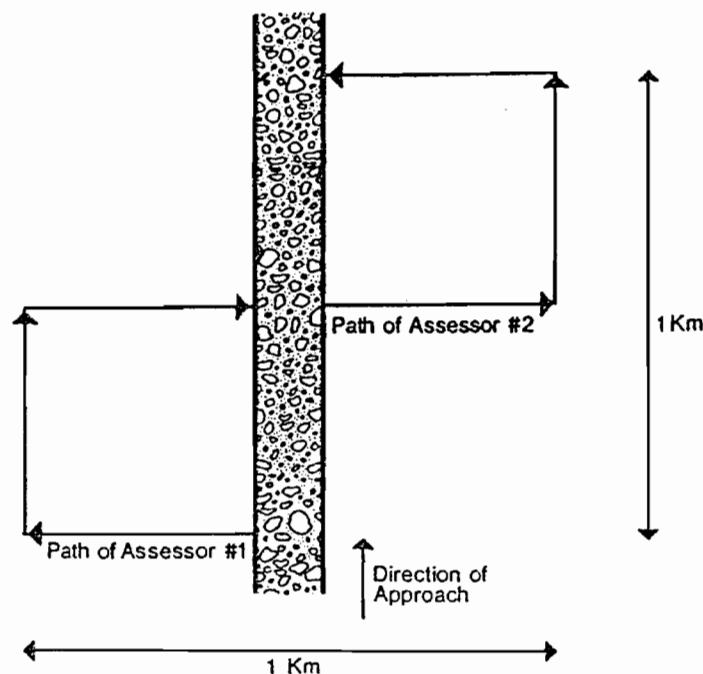


Fig. 8.6. Illustration of the ideal transect path followed by two assessors, beginning at a randomly selected location on the road. In practice, the assessors may vary from this path in order to encounter cultivation near the transect.

assumes that the 2-km stretch of road involved is relatively straight. Curves will cause the pattern of samples to be asymmetrical, which is permissible, although intervals containing sharp curves should probably be avoided.

This design for locating plots is not the same as that recommended and described earlier for use in large-scale surveys. The key difference is that the concept of a field as a primary sampling unit is not appropriate in this situation. The very small average size of a planting and the basically unknown distribution of these plantings precludes the use of a systematic system of parallel transects in each sampled 'field'. Note, however, that the present design involves locating sampling plots at systematic intervals and that straight-line transects are used, although they are not parallel. Also realize that the actual population being sampled does not include all cultivated land. Only the sorghum within 0.5 km of a road is eligible for sampling, and thus the inferences made from the data collected under this design pertain only to the collection of 1-km-wide strips centred on the area roads. If the investigator makes inferences to the entire area, he assumes that areas adjacent to roads are representative of the entire area. Obviously, this situation is less than ideal because all sorghum in the target area should have a chance to be sampled. The practicalities of the situation in Tanzania, i.e. lack of easy access to all agricultural lands, forced this compromise into our design.

Each plant within each sample circular plot is visually examined by the

assessor and assigned two values. The first value is a head size category, e.g. 1, 2, or 3, that indicates if the head is in a below average, average, or above average size class. The definition of these categories should be specified before the ground surveys are begun, and the assessors should be trained accurately to classify heads according to the defined size classes. The purpose of recording this variable is to provide some information about the quality of the assessed crop, and together with an estimate of plant density obtained from the average number of plants per plot, a crude index to the potential yield of the crop can be calculated. No direct information about the yield will be available, because it is simply too time-consuming and impractical to collect and weigh heads. Instead, the assessor records an estimate of the percentage of seed removed, in a 5 per cent incremented scale, using a visual examination of the head (Fig. 8.7). It must be remembered that the validity of the results of the entire survey ultimately depends upon the competence of the assessors.

Summarizing and analysing damage data are straightforward. The following notation is used to define the estimators. This notation does not distinguish between data collected by the two different observers in the same sampling location (1-km² block), i.e. the data from both observers are pooled for each location.

| | | | | | |
|---------------|-------------------|------------------------------------|--------------|----------------|------|
| Region | Singida, Tanzania | Date | 17 June 1985 | Beginning time | 0800 |
| Road location | 43 | Assessor | Otis | Ending time | 1100 |
| Plot Interval | 30 m | Direction of initial transect 240° | | | |

| Plot number | Size class | | |
|-------------|------------------|-------------------------|-------|
| | 1 | 2 | 3 |
| 1 | 0, 10, 20 | 0 | 0 |
| 2 | 15, 0, 0 | 5, 5, 5, 0, 40, 0, 0 | |
| 3 | 0, 10, 15 | 35, 50 | 15 |
| 4 | 0, 0, 0, 0, 0, 0 | | |
| 5 | | 10, 15, 25, 60 | 10, 5 |
| 6 | 0, 0 | 0, 10 | 0, 0 |
| 7 | 0, 15, 20 | 20, 20, 35 | |
| 8 | | 60, 40, 20, 0 | 0, 0 |
| 9 | 0, 0, 0 | | |
| 10 | | 0, 0, 0, 0, 0, 0 | |

Fig. 8.7. Example of a completed data form for recording bird damage within circular plots of 1 m radius. Each entry represents per cent loss estimated visually on a single head, and categorized by estimated size class of the head.

- Y_{ijk} = Estimated per cent loss of the k th plant in the j th plot at the i th sampling location, $i = 1, \dots, T, j = 1, \dots, R_i, k = 1, \dots, S_{ij}$,
 X_{ijk} = Size class of the k th plant in the j th plot at the i th sampling location,
 \bar{Y}_i =
$$\sum_{j=1}^{R_i} \sum_{k=1}^{S_{ij}} Y_{ijk} X_{ijk} / \sum_{j=1}^{R_i} \sum_{k=1}^{S_{ij}} X_{ijk}$$

= Weighted (by size class) average per cent loss of all plants assessed at the i th sampling location,
 \hat{Y} =
$$\sum_{i=1}^T \bar{Y}_i / T$$

= Estimated per cent loss in the region,
 $\text{Var}(\hat{Y})$ =
$$\sum_{i=1}^T (\bar{Y}_i - \hat{Y})^2 / T(T-1).$$

Note that, because of the weighting by size class, larger heads have greater influence in determining the overall loss. It is true that there is a slight statistical bias and potential loss of efficiency inherent in the estimator \hat{Y} , because the \bar{Y}_i have not been weighted by the relative amount of sorghum cultivation contained in the 1-km² areas sampled. The reason for this is simply that these amounts are unknown, i.e. the data collected by the assessors cannot provide an accurate estimate of the amount of cultivation in the area sampled. If the assessors were required to adhere strictly to the transect line, then such an estimate would be available by using the proportion of the transect length that intersected sorghum. However, it seems impractical to expend resources to get to a location and then not have the flexibility to collect a reasonably large sample, and it is this flexibility (allowing departure from the transect line) that invalidates an attempt to estimate the relative cultivation of the area. Thus, the choice was made to make the trade-off between obtaining weights and collecting a satisfactory number of samples at each location, with the assumption that the unweighted estimator \hat{Y} will be satisfactory in practice.

Bioenergetic models

Theoretical estimates of the amount of a specific crop likely to be consumed by a specific bird population can be produced by constructing a bioenergetic model for the particular ecological system involved. The bioenergetic requirements of each age and sex class in the population are estimated by creating a model that uses parameters such as digestive efficiency, temperature, population density and age structure, reproduction energy requirements, and produces a resulting population energy demand (Wiens and Dyer

1975). This output, combined with knowledge of the food habits of the species and the density and distribution of the crop, can then estimate the impact of the population on the crop. Wiens and Dyer (1975) used such an approach to estimate the amount of maize eaten by Red-winged Blackbirds in northern Ohio. Their energetics model was modified by Weatherhead *et al.* (1982) who were also interested in the impact of Red-winged Blackbirds on maize in Quebec. The authors also conducted a field study that measured actual consumption of maize by captive blackbirds during the damage season, and these figures were compared to their model predictions. The agreement between their model estimates and field data was quite good, although they differed markedly from estimates produced by the Wiens and Dyer energetics model.

Both studies emphasized the need for objectively evaluating the magnitude of the pest problem before further research or management strategies are put in place, and I am in full agreement with this attitude. They suggest that a modelling approach could replace large-scale damage surveys because it is less expensive and because of the highly variable estimates that often result due to the large variation in loss within the surveyed region. However, modelling also has its problems. First, the models require that values be specified for a large number of bioenergetic and pest population parameters, which may or may not be available. Second, the models are deterministic, so that a measure of the stochastic variation in the system, i.e. measures of variation in damage among fields or strata, are not available. These measures of reliability are valuable not only from a statistical point of view but can also be used as information concerning the distribution of loss within the region. Finally, of course, as with all modelling, the results will be subjective in the sense that no two modellers are likely to create the same model.

Despite these drawbacks, a model can be of value in that it requires the researcher to think in the context of an ecosystem with all its interactions and complexities, as opposed to a narrow focus that considers a pest and a crop isolated from the system. Also, if reliable estimates of parameters critical to the model are available from previous research, then bioenergetic estimates of loss may help to put the depredation problem in context. Although I am unfamiliar with the status of the data base involving quelea biology, I suspect that information of bioenergetic and population dynamics sufficient for the construction of reasonable model outputs does not presently exist. Considering the urgent need for information of problem definition on quelea, direct estimates of regional losses are presently more appropriate.

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